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VII.

ON THE TEMPERATURE OF THE SUN.

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IT is known to all that there is a problem of the highest interest in solar physics at present waiting solution: I mean that of the temperature of the sun; and, so far as the whole radiant energy is inferable from the rate of emission of heat, the problem is one the theoretical solution of which is evidently dependent on our knowledge of the laws of cooling.

Every operation of Nature, whether in the organic or inorganic kingdom, is accompanied by the emission or absorption of heat; and, considering that, whether the subject of observation be the germination of a seed, the heat of a stove, or the outflow from the sun upon the planetary system, we want to know the rate of the deperdition of energy, one might certainly suppose that no physical law would have been better ascertained; but we are here, however (at least in regard to high temperatures), in a state of nearly complete ignorance, and know almost literally nothing about what so intimately concerns us. This is a reproach to modern physics, which has probably made no real advance here since Newton. To justify this language, I remark that, in the case of the solar temperature, the *amount* of heat the sun sends us is scarcely in question, as we are all substantially agreed on the way to measure this and on the results of measurement. The latest of these give, it is true, larger values than those of Pouillet, which were about 1.75 calories per centimeter per minute instead of 2.50; but these considerable variations are so trifling compared with those in the deductions made from them, that we may still say there is substantial agreement as to data. From like data, then, Sir John Herschel concludes that the temperature of the solar surface is over 5,000,000° centigrade; Mr. Ericsson, whose labors on this point deserve wider recognition, is confident that the temperature is

not materially different from 4,000,000° Fahrenheit; Father Secchi, in his latest research, makes it 133,000° C.; Sir Wm. Thompson and others estimate 30,000 to 60,000° C.

These extremely gross discrepancies having drawn general attention, many distinguished French physicists have lately reinvestigated the subject, and, using Dulong's and Petit's formula, have after most elaborate research arrived at the nearly unanimous conclusion that the temperature of the solar surface is altogether lower than any of these, — is in any case not more than 2000 to 2500° C, but is more probably below than above the temperatures which are reached in our furnaces, and in fact is probably less than that of melting platinum.

It is here to be borne in mind that we really know nothing about the absolute emissive capacity of the solar surface, and that to simplify the problem, when we speak of the sun's being at a lower temperature than that of a certain lamp-black surface or hot platinum or steel, it is assumed for the purpose of comparison, by myself as well as by the above-named investigators, that both the solar and terrestrial sources of heat have the same emissive capacity. The temperature thus defined has been called the "effective" temperature.

M. Violle, one of the most distinguished students of the subject, whose experiments bear evidence of intelligent care, found by observations at Grenoble, in March, 1874, that, with an emissive power thus defined, the temperature of the solar surface was 1230° C.* In a subsequent memoir, he finds for the same the rather higher value of 1354° C.† After allowing for absorption in our atmosphere, it remains true that the temperature is then much below that of melting platinum, and this seems to be confirmed by his later results, which give about 1550° C. as the highest "effective" temperature.

All these and other observations involve the use of the empirical formula, well known as that of Dulong and Petit, which has replaced the earlier and simpler one of Newton.

Now, whatever be the apparent presumption of opposing my opinion to that of so many conscientious and recent investigators, I feel there is something yet to be said; and the present paper is an account of experiments of a special character, undertaken at the expense of the Rumford fund, not with the hope of at once solving so arduous a problem, but with the wish, in this confusion of opinion, to contribute one or two incontrovertible facts as material towards the con-

* Comptes Rendus, vol. lxxviii. p. 1425.

† C. R. vol. lxxviii. p. 1816.

struction of future theory. I hope to show convincingly that the sun's "effective" temperature is at any rate far above that of any ordinarily attained in the arts (very much above that of melting platinum for instance), and incidentally that the law of Dulong and Petit is untrustworthy precisely where we need to apply it.

If we have no formula by which to infer the temperature of the sun, there remains the comparison of its radiation with that of a terrestrial source of high *known* temperature. Thus the late Father Secchi has measured the radiation from the electric arc, and M. Violle that from a Siemens-Martin's furnace; but, by comparing these only with others made at other times on the sun, discrepant results appear also. Were we, however, to compare the sun *directly* with a terrestrial source of high temperature, and bringing them face to face find one giving more heat than the other, there could (with equal emissive powers) be no question but that the body radiating more heat was also the higher in temperature. Strange to say, this simple test has never, that I know, been applied to this problem * until in the experiments I am about to describe.

We have in the arts one process which gives what we want ready to hand in the production of a vertically disposed surface of several square feet of a liquid metal, hotter than melted platinum itself. I refer to the Bessemer process now in use in several places in this country, among others at the Edgar Thompson steel-works about twelve miles from Pittsburg. I have received every possible assistance from the managers of this great establishment, and owe my acknowledgments here for their kindness.

As the Bessemer process may be as vaguely known to some as it was till lately to me, I will first briefly describe so much of it as concerns the present purpose.

An enormous egg-shaped vessel called the "Converter," capable of holding 30,000 to 40,000 pounds of melted metal, is swung on trunnions so that it can be raised by an engine to a vertical position, or lowered so as to pour its contents into a caldron. First, the empty "converter" is inclined, and into its mouth is poured about 15,000 pounds of fluid pig-iron, whose temperature as it flows in from an adjacent furnace, where it has previously been melted, is about 1400° C. Then the "converter" is lifted to an erect position, and an air-blast from a powerful blowing-engine is forced up through its liquid con-

* Experiments with the lime and electric lights made for other purposes are not here in question.

tents. In the 15,000 pounds of impure iron there are ordinarily found about 230 pounds of silicon and 540 of carbon; and as each pound of carbon gives 8000 calories, and each pound of silicon 12,000 to 14,000, in connection with the air-blast's unlimited supply of oxygen, the temperature of the already molten metal rapidly rises under this enormous inflow of several million calories of heat. After the blast has continued eighteen to twenty minutes, the temperature of the contents is from 1800° to 2000° C., or higher than that of melted platinum, taking the lowest estimate; and now the "converter" is again lowered, and about 1500 pounds more of melted iron added. The temperature here perhaps falls slightly, but its effect may be judged by any one who sees this second lot of iron poured in. Melted iron by itself, every one knows, seems dazzlingly bright; but as this streams into the open mouth, the interior is so much brighter still, that the stream is deep-brown by comparison, presenting a contrast like that of dark coffee poured into a white cup. The contents are now no longer iron, but liquid steel ready for pouring into the caldron; and, looking from in front into the inclined vessel, we see the almost blindingly bright interior dripping with the drainage of the metal running down its sides, so that the circular mouth, which is twenty-four inches in diameter, presents the effect of a disk of molten metal of that size, were it possible to maintain such a disk in a vertical position. In addition, we have the actual stream of falling metal which continues nearly a minute, and presents an area of some square feet. The shower of scintillations from this liquid cataract of what seems at first "sun-like" brilliancy, and the immense area whence such intense heat and light are for a brief time radiated, make the spectacle a most striking one.

Platinum dipped in the steel as it pours from the lip melts away; and not to rely on this evidence, which might be alleged to be due to an alloying rather than a true melting, I procured some platinum wire which Mr. Preusser, the chemist of the works, stretched at my request across the open mouth of the "converter" when in an erect position. The platinum, here several yards above the metal, was melted by the blast which came from it.

Heat Comparisons.

After many visits to the works, much trouble and repeated failures due to the difficulties of working in such novel circumstances, I secured a series of trustworthy measures, in May last, both of heat and light.

I describe my apparatus here in principle, not in detail; and I omit many preliminary experiments, as well as some minute corrections applied for small instrumental errors, giving my results in general terms. One difficulty attending a simultaneous comparison was to obtain a station looking into the "converter" at the time it was inclined and pouring, and yet necessarily outside the building in the sun-light. To do this, I stood in a window (whence the sash had been removed) of the west wall, sixty-one feet from the "converter" mouth. A platform was erected here for my apparatus, part of which was clamped to the wall itself; but though this was the best point of observation, the noise, the shower of sparks driven over the instruments from within by the blast at each "pour," and the rain of wet soot without which fell thick at times on apparatus and observer from the combined steam and smoke of adjacent chimneys, made the task of observation another thing from what it is in the quiet of a physical cabinet.

From this window-station, a *porte-lumière* reflected the sun's rays, so that traced through the dusty air the beam was seen to enter the "converter" mouth, or fall on the stream which ran from it. In the path of this beam was a cylinder, containing within a double enclosure an Elliott thermopile of forty small elements, similar to that I had used for some years on the sun, and surrounded by all the precautions against air-currents and extraneous influences taught me by experience. The pile exposed both faces at once, one to the furnace, the other to the reflected sunbeam; and a Thompson reflecting galvanometer read by an assistant, and placed at a considerable distance from any moving iron, gave prompt evidence as to which face was hotter.

The angular area, subtended at the pile by the fluid metal, was always many times that subtended by the sun's disk, and there was no lens or medium of any kind (except air) between the "converter" mouth and the pile. Supposing, then, the metal to have only presented a disk equal in angular diameter to that of the sun, if the needle remained stationary, it is plain that each was sending an equal amount of heat, and that any square foot of the solar surface was radiating at least as much heat as a square foot of the metal; for it is obvious that the distances of the two sources have nothing to do with this effect under the given conditions.

The metal area, however, being many times that of the sun, the latter still over-balanced the metal; showing that the sun was actually very much the hotter. Accordingly, there was interposed between the *porte-lumière* and the pile a telescope which diffused the sun-light

over an image of any given diameter. As the solar light entered only through a diaphragm of known dimensions, it was easy to say how much the sun's heat was weakened to balance that from the metal. It must be borne in mind, however, that there was no account taken of the loss of solar heat by reflection and absorption in the lenses, by reflection from the mirror, and more than all by the frequent clouds of smoke and steam, while the furnace heat suffered no diminution whatever. Further, every other condition of the experiment was designedly such as to weigh in favor of the furnace and against the sun's heat. The value found for the latter, then, is a minimum value. I should perhaps have remarked that experiments had shown that the trifling heat from objects near the melted metal might be neglected. That from the atmosphere about the sun was also insignificant. Except, then, for the diminution of solar heat by absorption, reflection, and so on, our method is equivalent to bringing a specimen piece from the sun's surface (if I may so express myself) face to face with one from the furnace, placing our thermopile mid-way between them, and determining how much we have to diminish the size of the former to make its heat-radiation no more than equal the latter's.

The result of these experiments was that the minimum value we can assign to the solar radiation is eighty-seven times that from an equal area of the pouring metal. This, it will be remembered, is not an actual but a minimum value. The true value may be indefinitely greater.

Part Second.

Photometric Comparisons.

Of the complex radiations from any source of high temperature, a part is interpreted by the pile as heat, a part by the eye as light; but as the temperature is raised, it is now well known that the waves of shorter length increase in amplitude much faster than the longer ones. If the temperature of the sun, then, be much greater than that of the furnace, we shall have a quite independent proof of the fact in a photometric comparison, which, we can safely pronounce *a priori*, will then give a very much greater ratio of sunlight to furnace-light than that of sun-heat to furnace-heat. To make this comparison, a photometer box, about 8 inches in square section and 66 inches in length, is placed so that its central axis lies as before in the path of the reflected beam from the mirror to the furnace. Two similar

telescopes of 1.66 inch aperture and 20.01 inches focus, having their objectives outside the extremities of the box and their optical axes in the path of the beam, project, by their eye-pieces, images of the sun and of the pouring metal on the two sides of a Bunsen disk, whose normal position is in the centre of the box. Both images are viewed simultaneously by mirrors attached to the disk, which is movable along a graduated scale. (I here omit certain small corrections applied in practice, and describe the use of the instrument in brief terms.) We do not now need to consider the relative angular areas of the sun and furnace, for so long as both are of appreciable size the images of both falling on the screen, when nearly midway between the two telescopes, will be sensibly proportioned in brightness to the absolute intensities of the sun-light and furnace-light. We do in fact, however, at the outset find the sunlight so immensely brighter that no direct comparison is possible. We then diminish the aperture of the solar telescope (which we will call A), till it has a small known ratio to that of the furnace telescope (which we will call B). In practice B was always left with the full aperture of 1.66 inch diameter, while that of A was 0.192. Were the original sources of equal intensity, the sunlight would have been reduced to $\left(\frac{0.192}{1.660}\right)^2 = 0.013 +$ or a little over one one-hundredth of the other. But it was surprising to see that the image from A was even now incomparably stronger than that formed either by the flame from the blast at its brightest, or by the pouring metal. Under these circumstances, the Bunsen disk was moved from its central position toward B, thus approaching the apex of one light cone and withdrawing from the other, so as to diminish the sunlight still further in an exactly determinable ratio. The lowest value obtained in a series of accordant measures gave intensity of sunlight over (5300) five thousand and three hundred times that from the metal; and this value is, I think, considerably below the truth.

It results from these experiments: (1) That direct observation disproves the statement that the sun's effective temperature does not exceed 1500° C. It is demonstrably over 1800° C., and may for any thing here shown to the contrary be indefinitely greater.

(2) The solar heat-radiation, so far from being comparable to furnace heat, is at a minimum something like 100 times that from melted platinum, area for area, and probably much greater.

(3) The solar-light radiation (which offers a more trustworthy indication of the total difference between the sum of all degrees of radiant energy than the heat) is over 5300 times that from a temperature above that of melted platinum.

(4) Since all the above results are simple statements of the facts of experiment, and are independent of formulas, we conclude that the formula of Dulong and Petit (which from well-conducted experiments, like those of M. Violle, deduces conclusions which trial disproves) must be itself wrong. Further, since this formula contains no term depending on the wave-length, it takes no account of the difference here proved to exist between the relative quantities of heat and light radiation from sources of high temperature, and is thus found especially untrustworthy at those temperatures at which it has been most frequently applied.

I do not yet venture an opinion of my own on the real temperature of the sun, further than that I think it much higher than has been of late believed.

The preceding observations and inferences all seem to point to the use of the highest attainable terrestrial temperatures (*e. g.* that of the electric light) in comparisons (and the consequent least dependance on formulas) as the safest line for future investigation.

Pl. I.

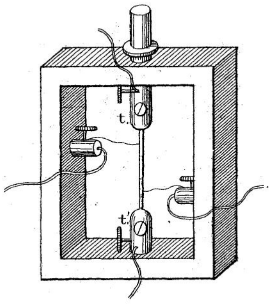

Fig. 2.

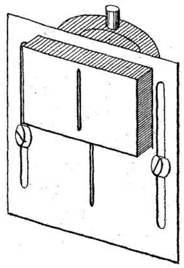

Fig. 3.

Fig. 1.